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THE FUNDAMENTAL LAWS OF ARITHMETIC.[1]

[INTRODUCTORY NOTE.

Friedrich Ludwig Gottlob Frege was born on November 8, 1848, at Wismar, and since 1874 has taught at the University of Jena. It is well known that there has been in mathematics, especially during the last century, a constantly growing tendency toward more rigorous proofs and a more accurate determination of the limits of validity of mathematical propositions. For these purposes accurate definitions of mathematical concepts were needed; thus we obtained a much greater distinctness in definitions of a function, of the limits and continuity of a function, of the infinite, and of negative and irrational numbers. A natural continuation of this path of research led to the investigation of the question whether the concept of whole number is capable of definition, and whether the simplest laws which hold for integers are capable of proof. This we see in the work of Cantor, Dedekind, and Frege.[2] The object of a proof, as Frege has said, is not merely to raise the truth of a proposition above every doubt, but also to impart an insight into the dependence of truths on one another. The farther these investigations are continued, the fewer will be the fundamental truths to which everything can be reduced; and this simplification is in itself an end worthy to be striven for.

It was this desire for simplification, together with the philosophical questions as to the *a priori* or *a posteriori*, synthetic or analytic, nature of arithmetical truths, which moved Frege to his investigations. According to Frege, if in our proofs of mathematical truths we only meet the laws of logic and definitions we have an "analytic" truth, but if it is not possible to carry out the proof

[1] Translated, with the exception of the Introductory Note, from Professor Frege's *Grundgesetze der Arithmetik* by Johann Stachelroth and Philip E. B. Jourdain.

[2] It may be mentioned here that the Open Court Publishing Company of Chicago and London has issued translations of the most important work of Dedekind (*Essays on the Theory of Numbers*) and Cantor (*Contributions to the Founding of the Theory of Transfinite Numbers*) in this direction.

without using principles which are not, in general, of a logical nature, but refer to a special domain of knowledge, the theorem is "synthetic." This distinction of Frege's is not quite Kant's distinction, but it is an extension of Kant's more limited view that the sole source of analytic judgments is the principle of contradiction. "If," said Frege, "we call a theorem *a posteriori* or analytic, we do not judge about the psychological, physiological and physical conditions which made it possible to form the content of the theorem in our consciousness, nor about how another person has—perhaps in an erroneous manner—arrived at maintaining its truth, but about the ultimate foundation of the justification for the maintenance of its truth.... A truth is *a posteriori* if its proof must depend on facts, that is to say, unprovable truths without generality that contain statements about definite objects. If, on the other hand, it is possible to carry out the proof wholly from general laws which themselves neither are capable of proof nor need it, the truth is *a priori*."[3] Of the four combinations, then, between analytic and synthetic on the one hand and *a priori* and *a posteriori* on the other, one only—analytic *a posteriori*—drops out.[4]

A still earlier account, written by Frege, is that he proposed to himself the question as to whether arithmetical judgments can be proved in a purely logical manner or must rest ultimately on facts of experience. Consequently he began by finding how far it was possible to go in arithmetic by inferences which depend merely on the laws of general logic. In order that nothing that is due to intuition should come in without being noticed, it was most important to preserve the unbrokenness of the chain of inferences; and ordinary language was found to be unequal to the accuracy required for this purpose.

Hence arose what Frege called his *Begriffsschrift*— a word that may be translated as "ideography"—which was described and shown in use in a small book published at Halle in 1879 under the title: *Begriffsschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens.* The fundamental idea of this book was the transference of the distinction of "variable" and "constant" from mathematical analysis to the wider domain of pure thought in general. In mathematics the distinction is not thoroughly carried out; but Frege's distinction was quite thorough.

[3] *Grundlagen der Arithmetik,* Breslau, 1884, pp. 3-4.
[4] *Ibid.,* p. 17.

He divided all the signs that he used into: (1) letters, "by which we can represent to ourselves different things," like those in the generally valid theorem in mathematics $(a+b)c=ac+bc$ and which serve principally to express *generality*; (2) signs which have quite a definite meaning, like $+$, $-$, 0, 1, or 2.

We have seen that arithmetic was the starting-point on the road that led Frege to his ideography. The aim of this ideography was not to provide a means of dealing systematically and rapidly with complicated logical questions, but to enable the question as to the empirical or purely logical basis of a branch of knowledge—in this case arithmetic—to be finally settled.

Frege began his *Begriffsschrift* by pointing out that, when we raise the question as to the foundation of a truth, the answer which —unlike that given by the recounting of the historical genesis and development of our knowledge of the truth in question—is connected with its inner being, consists in carrying out its proof purely logically, if that is possible, or, if it is not, in reducing it to the facts of experience on which the proof rests. The firmest proof is obviously a purely logical one, which, abstracting as it does from the special nature of things, is founded wholly on the laws on which all knowledge rests. Certainly a proposition may be capable of logical proof and yet could never, without sense-perception, enter into our consciousness. Indeed, this seems to be the case with *every* judgment, since no mental development without sense-perception appears possible. Thus it is not the psychological origin, but the completest manner of proof, that brings about the division of the class of all truths which need founding into (a) those which can be proved purely logically, and (b) those whose proofs rest on facts of experience.

And the very important ends for which Frege's ideography was designed were more or less overlooked by Venn, Schröder, and Peano, who criticized principally the cumbrousness of Frege's notation. This cumbrousness is a fact, but it may, as Bertrand Russell has shown, be avoided to a great extent. Far more important than the awkwardness of the form of many of the symbols, however, is the subtle and profound analysis of the ideas of logic, and the perfect avoidance of ambiguity and implicit assumptions. These are the most prominent characteristics of Frege's work.

In the following translation of part of Frege's mature exposition of 1893 and 1903, any notes or references which have been

added by myself are put in square brackets. It is to be hoped that the present translation, for which Professor Frege has most kindly given me his permission, will help to make Frege's magnificent work better known. Frege's work is the first of that of the modern logicians. Mr. Bertrand Russell in his "Lowell Lectures" of 1914,[5] has given a notable example of the "logical-analytic" method in philosophy of which "the first complete example is to be found in the writings of Frege," and this method is now becoming almost as widely known as its importance deserves.—P. E. B. J.]

THE ideal of a strictly scientific method in mathematics, which I have tried to realize here and which perhaps might be named after Euclid, I would like to describe in the following way.

It cannot be expected that we should prove everything, because that is impossible; but we can demand that all propositions used without proof should be expressly mentioned as such, so that we can see distinctly upon what the whole construction is founded. We should, then, strive to diminish the number of these fundamental laws as much as possible by proving everything that can be proved. Furthermore I demand—and that is where I go beyond Euclid—that all the methods of inference used must be specified in advance. Otherwise it is impossible to satisfy the first demand.

At this ideal I believe I have arrived in essentials: only in a few points could one possibly be more exacting. In order to assure myself of more freedom and in order not to drop into excessive prolixity, I have taken the liberty of making tacit use of the interchangeability of the minor terms (conditions) and of the possibility of amalgamation of identical minor terms, and I have not reduced the modes of inference to their smallest number. Those who have read my *Begriffsschrift* will be able to gather from it that it

[5] *Our Knowledge of the External World as a Field for Scientific Method in Philosophy,* Chicago and London, 1914; cf. p. v.

would even in this respect be possible to satisfy the severest demand, but that it would at the same time involve a considerable increase in volume.

I believe that, apart from this, the only objections which could justly be raised to this book do not concern the rigor but only the choice of the course of the proofs and of the intermediate steps of the proofs. Often there are several modes of proof possible; I have not tried to adopt them all, and thus it is possible—even probable—that I have not always chosen the shortest. But let whoever has any fault to find with regard to this do better himself. There are other matters about which it is possible to dispute. Some might have preferred to increase the number of the modes of inference admitted and thereby to arrive at a greater mobility and brevity. But we have to stop somewhere if my ideal is approved of, and wherever we stop, people may say: "It would have been better to admit still more modes of inference."

By the uninterrupted connection of the chains of inference, each axiom, assumption, hypothesis, or whatever we like to call it, upon which a proof is founded, is brought to light, and so we gain a basis for judgment on the epistemological nature of the law proved. It has often been said that arithmetic is only a more highly developed logic; but that remains disputable as long as the proofs contain transitions from one proposition to another which are not performed according to acknowledged logical laws, but seem to be founded on intuitive kowledge. Only when these transitions are resolved into simple logical steps can we be sure that arithmetic is founded solely upon logic. I have gathered together everything that can facilitate the judgment as to whether the chains of inference are convincing and the buttresses firm. If any one perchance finds anything faulty, he must be able to indicate exactly where, to his thinking, the error lies—whether in the fun-

damental laws, in the definitions, in the rules, or in their application at a definite place. If we find everything correct, we know thus the exact bases upon which each single theorem is founded. A dispute can only, as far as I can see, arise because of my fundamental law of "ranges" (*Werthverläufe*),[6] which perhaps has not yet been specifically expressed by logicians, though it is in their minds when, for example, they speak of extensions (*Umfänge*) of concepts. I hold that it is purely logical. In any case the place is indicated where the decision has to be made.

My purpose requires many deviations from what is usual in mathematics. The requirements with regard to the rigor of proofs inevitably entail a great length of these proofs. Whoever does not think of this will often be surprised at the roundabout way in which a proposition is here proved, whereas he believes he can grasp the proof directly by a single act of understanding. This will surprise us especially if we compare the work of Dedekind, *Was sind und was sollen die Zahlen?*,[7] which is the most thorough work on the foundation of arithmetic that I have lately seen. In a much smaller compass it follows the laws of arithmetic much farther than I do here. This brevity is only arrived at, to be sure, by much not being really proved at all. Dedekind often says only that the proof follows from such-and-such theorems; he uses little dots which have the vague meaning of "and so on";[8] nowhere is there a statement of the logical or other laws on which he builds, and, even if there were, we could not possibly find out whether really no others were used,—for

[6] [This theorem is numbered V on pp. 36 and 240 of Vol. I (1893) of the *Grundgesetze*; and expresses that an equality of ranges both implies and is implied by the statement; thus an equation between functions holds quite generally. It first appeared on page 10 of Frege's lecture, *Funktion und Begriff* (Jena, 1891). Cf. p. 253 of Vol. II of the *Grundgesetze* (1903).]

[7] [English translation on pp. 29-115 of Dedekind's *Essays on the Theory of Numbers* (Chicago and London, 1901).]

[8] [Cf., for example, paragraph 8 on page 47 of the above translation.]

to do that the proof must be not merely indicated but completely carried out. Dedekind is also of the opinion that the theory of number is a part of logic; but his work hardly contributes to strengthen this opinion, because the expressions "system" and "a thing belongs to a thing" used by him are not usual in logic and are not reducible to accepted logical doctrine. I do not say this as a reproach, for his method may have been the most serviceable to him for his purpose; I only say it to make my intentions clear by putting them by the side of opposite intentions. The length of a proof is not to be measured by the yard. It is easy to make a proof appear short on paper by omitting many connecting links in the chain of inference and only indicating many things. Generally we are satisfied if every step in the proof is seen to be correct, and we may be so if we intend to arouse conviction of the truth of the theorem to be proved. If we wish to bring about an insight into the nature of this perception of the truth this method does not suffice, but we must put down all the intermediate stages of reasoning, in order that the full light of consciousness may fall upon them. As a rule mathematicians are only interested in the content of a theorem and in the fact that it is to be proved. The novelty of this book does not lie in the content of the theorems but in the development of the proofs and the foundations upon which they are based. That this altogether different point of view needs a quite different treatment ought not to appear strange. If we deduce one of our theorems in the usual way, it will be easy to overlook a proposition which does not appear necessary for the proof. If my proof is carefully thought out, the indispensability of this proposition will, I believe, be seen, unless an altogether different mode of procedure is adopted. Thus perhaps there are here and there in our theorems conditions which appear at first to be unnecessary but which after all prove to be necessary or at least to

admit of removal only by a proposition to be specially proved.

With this book I accomplish an object which I had in view in my *Begriffsschrift* of 1879 and which I announced in my *Grundlagen der Arithmetik*.[9] I will here substantiate the opinion on the concept of number that I expressed in the book last mentioned. The fundamental part of my results is there expressed in § 46 in the words that the numerical datum contains an assertion about a concept; and upon this my present work is founded. If anybody is of another opinion let him try to construct a logical and usable exposition of his view by signs, and he will see that it is impossible. In language, it is true, the state of affairs is not so obvious, but if we look into the matter closely we find that here too a numerical datum always denotes a concept, not a group, an aggregate[10] or such-like things; and that if a group or aggregate is named, it is always determined by a concept, that is to say, by the properties an object must have in order to belong to the group, while that which makes the group a group or the system a system —the relations of members to each other—is altogether indifferent for the number.

The reason why the demonstration appears so long after the enunciation is to be found in part in essential changes of my ideography, which have forced me to discard a manuscript that was almost completed. These improvements may be mentioned here briefly. The fundamental signs employed in my *Begriffsschrift* have with one exception been used again here. Instead of the three parallel lines I have chosen the ordinary sign of equality because I convinced myself that it has exactly the same meaning in arithmetic that I wish to designate. I use the ex-

[9] Cf. the introduction and §§ 90 and 91 of my *Grundlagen der Arithmetik*, Breslau, 1884.

[10] [However, "aggregate" has become a technical term in mathematics for the translation of *"Menge"* or *"Begriffsumfang,"* and not *"Aggregat."*]

pression "equal" in the same sense as "coinciding with" or "identical with," and this is just how the sign of equality is really used in arithmetic. The objection which might perhaps be raised against this rests on a defective distinction between sign and what is signified. It is true that in the equation $2^2 = 2 + 2$ the sign on the left is different from the one on the right, but both indicate or denote the same number.[11] To the old fundamental signs two more have been added: the "smooth breathing" (*spiritus lenis*) which serves for the designation of the "range" (*Werthverlauf*) of a function, and a sign which is meant to take the place of the definite article of ordinary language. The introduction of the ranges of functions is an important advance which makes possible a far greater flexibility. The former derived signs can now be replaced by other and simpler ones, though the definitions of one-to-one-ness, of a relation, of succession in a series, and of representation (*Abbildung*) are essentially the same as those which I have given partly in my *Begriffsschrift* and partly in my *Grundlagen der Arithmetik.* But the ranges have also a great fundamental importance; in fact I even define number itself as the extension of a concept, and extensions of concepts are, according to my definition, ranges. In consequence, we cannot do without them. The old fundamental signs, which reappear outwardly unchanged and whose algorithm has also hardly changed, have nevertheless been supplied with other explanations. The former "line of content" (*Inhaltsstrich*) reappears as a horizontal line (*Wagerechter*). There are consequences of an energetic development of my logical views. Formerly I distinguished in that proposition whose outer form is an assertion two things: (*a*) The recognition of the truth; (*b*) The content which is recognized as true. The content I called the "judicable content" (*beurtheil-*

[11] I also say: the meaning (*Sinn*) of the sign on the right is different from that of the one on the left but the denotation (*Bedeutung*) is the same (*Zeitschr. für Philos. und philos. Kritik,* Vol. C, 1892, pp. 25-50).

barer Inhalt). The latter has been divided into what I call "thought" (*Gedanken*) and "truth-value" (*Wahrheitswerth*). That is a consequence of the distinction between the meaning and denotation of a sign. In this case the meaning of a proposition is the thought and its denotation the truth-value. Besides this, we must grant that the truth-value is the true. I distinguish two truth-values: the true and the false. This distinction I have discussed more exhaustively and substantiated in my above mentioned essay on meaning and denotation. It may be mentioned here that incorrect speech can only thus be rightly understood. The thought which is otherwise the meaning of a proposition becomes, in incorrect speech, its denotation. How much more simple and distinct everything becomes by the introduction of truth-values can only be seen by an exhaustive examination of this book. These advantages alone put a great weight into the balance in favor of my view, which view perhaps may seem strange at first sight. Also the essence of the *function* in contradistinction from the *object* (*Gegenstand*) is more distinctly accentuated than in my *Begriffsschrift*. From this results further the distinction of function of the first and second "stage" (*Stufe*). As I have shown in my essay *Funktion und Begriff*, published at Jena in 1891, relations are functions in the meaning of the word which has been extended by me, and so we have to distinguish concepts of the first and second stage, relations of the same and of different stages.

From this it will be seen that the years have not passed in vain since the appearance of my *Begriffsschrift* and *Grundlagen*: they have brought my work to maturity. But just that which I recognize as an important advance stands, as I cannot help seeing, as a great obstacle in the way of the circulation and effectiveness of my book. And the strict completeness of the chain of conclusions, which seems to my way of thinking not its least value, will bring

it, I am afraid, little thanks. I have got farther away from the traditional ideas and have by so doing given an appearance of paradox to my views. An expression which is encountered here and there on rapidly turning over these pages may easily appear strange and produce an unfavorable impression. I myself can judge somewhat with what opposition my innovations will be met because I have had to overcome something similar in myself. For not at random or because of the desire for innovation did I arrive at them, but I was forced by the matter itself.

With this I arrive at the second reason for my delay: the discouragement which at times came over me because of the cool reception, or rather the want of reception, by mathematicians,[12] of my works mentioned above and the opposing scientific currents against which my book would have to fight. Even the first impression must frighten people away: unknown signs, pages of nothing but strange-looking formulas. It is for that reason that I turned at times toward other subjects. But I could not keep the results of my thinking which seemed valuable to me myself locked up in my desk for any length of time; and the labor I had spent always required renewed labor that it might not be in vain. So the subject did not let go its hold upon me. In a case like the present one, when the value of a book cannot be recognized by a hasty perusal, criticism ought to be a help. But criticism is generally too badly paid. A critic can never hope to get paid in cash for the pains which the thorough study of this book will cost him. The only remaining hope is that somebody may have beforehand sufficient confidence in the matter to expect that the subjective gain will be sufficient recompense, and that

[12] In vain do we seek a notice of my *Grundlagen der Arithmetik* in the *Jahrbuch über die Fortschritte der Mathematik.* Investigators in the same domain, Dedekind, Otto Stolz, and von Helmholtz, do not seem to know my works. Nor does Kronecker mention them in his essay on the concept of number.

he will then publish the results of his searching examination. It is not as if only a laudatory review would satisfy me: quite the contrary. I would by far prefer an attack supported by a thorough acquaintance with the subject than to be praised in general terms which do not touch the root of the matter....

I must give up hope of securing as readers all those mathematicians who, when they come across logical expressions like "concept," "relation," "judgment," think: *Metaphysica sunt, non leguntur*; and those philosophers who at the sight of a formula call out: *Mathematica sunt, non leguntur*. Perhaps the number of these people is not very small. Perhaps also the number of mathematicians who trouble themselves about the foundation of their science is not great, and even those who do often seem in a great hurry to get past the foundations. And I hardly dare hope that my reasons for laborious rigor and consequent lengthiness will convince many of them. As we know, what is long established has great power over the minds of men. If I compare arithmetic with a tree which develops at the top into a multitude of methods and theorems while the root pushes downward, it seems to me that the pushing of the root is, at least in Germany, rather weak. Even in a work which might be classed among those dealing with foundations, the *Algebra der Logik* of E. Schröder, the top-growth soon predominates and, even before a great depth has been reached, causes a bending upward and a development into methods and theorems.

The widespread inclination to recognize only what can be perceived by the senses as existing is also unfavorable for my book. It is sought to deny, or at least to overlook, what cannot be thus perceived. Now the objects of arithmetic, that is to say numbers, are of a kind which cannot be thus perceived. How are we to deal with them? Very simply: the signs used for the numbers are explained to

be the numbers themselves. Then in the signs there is something visible, and that is the chief thing. No doubt the signs have altogether different properties from the numbers themselves, but what does that matter? We simply ascribe to them the desired properties by means of what we call definitions. How on earth there can be a definition where there is no question about connections between sign and what is signified by it is a puzzle. We knead together sign and what is signified as far as possible without making any distinction between them, and, according to circumstances, we can assert the existence of the result with mention of its tangibility,[13] or we can bring into prominence the actual properties of numbers. Sometimes these number-signs are, it seems, regarded as chessmen and the so-called definitions as rules of the game. The sign then does not signify anything, but is the subject-matter itself. It is true that in this we overlook one little thing: that is, that we express a thought by $3^2 + 4^2 = 5^2$, while a position of chessmen does not express anything. Where people are satisfied with such superficialities, there is of course no basis for a deeper understanding.

Here it is of importance to make clear what defining is and what we can reach by it. It is, it seems, often credited with a creative power while really all there is to defining is that something is brought out, precisely limited and given a name. The geographer does not create a sea when he draws border lines and says: The part of the surface of the water surrounded by these lines, I am going to call the Yellow Sea; and no more can the mathematician really create anything by this process of definition. Nor can we by a mere definition magically give to a thing a property which it has not got. All we can do is to call

[13] Cf. E. Heine, "Die Elemente der Funktionslehre," Crelle's *Journal für Math.*, Vol. LXXIV, p. 173: "I place myself in my definition in a purely formal position and call certain tangible signs numbers so that in consequence the existence of these numbers is not in question.

this particular property by a new name. But that an oval drawn on paper with pen and ink should acquire by definition the property that when it is added to one one results, I can only regard as a scientific superstition. One might just as well make a lazy pupil diligent by a mere definition. Confusion easily arises here through lack of a distinction between concept (*Begriff*) and object (*Gegenstand*). If we say: "A square is a rectangle in which the adjacent sides are equal," we define the concept *square* by indicating what properties something must have in order to fall under this concept. I call these properties "characteristics" (*Merkmale*) of the concept. But it must be carefully noted that these characteristics are not the properties of the concept. The concept *square* is not a rectangle, only the objects which fall under this concept are rectangles, just as the concept *black cloth* is neither black nor a cloth. Whether or not such objects exist is not immediately known by means of their definitions. Now, for instance, suppose that we wish to define the number zero by saying: "It is something which when added to one gives one." With that we have defined a concept by stating what property an object must have to fall under this concept. But this property is not a property of the concept defined. It seems that we often imagine that we have created by our definition something which when added to one gives one. This is a delusion. Neither has the concept defined this property, nor is the definition a guarantee that the concept is satisfied. That requires first of all an investigation. Only when we have proved that there exists one object and one only with the required property are we in a position to give this object the proper name "zero." To create the zero is consequently impossible. I have already repeatedly explained this but, as it seems, without result.

JENA, GERMANY. GOTTLOB FREGE.